Reply on RC2
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Author comment on "Celestial Mechanics and Estimating the Termination of the Holocene"
by John Abele Parmentola, EGUsphere,
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The first issue here is terminological, but is indicative of major problems throughout the paper:

In mathematics and physics including geophysics, generally, "convolution" and "deconvolution" refer to a specific mathematical operation, equivalent to multiplying and dividing fourier transforms. The author seems to think a "deconvolution" model means an "explanatory" model, but if there is any deconvolution or convolution going on here, I've completely missed it. The "model" is a quasi-instantaneous linear forcing represented through the simple Eq. 3 with no reference to the physics or chemistry of the climate system.

I do not understand what the reviewer means by an explanatory model. Deconvolution is used in the sense of partitioning two effects associated with insolation, namely the contribution from the obliquity and the other from the precession index (the precession modulated by the eccentricity). It is not immediately obvious that such a partitioning is quantitatively valid, but systematic computations presented in the paper bear this out as represented by Figures 7 and 8.

This partitioning is performed in temporal space unlike the typical frequency space analyses. There are physical reasons for this choice. The first has to do with the EDC data, which is represented as a time series. As I discuss in the introduction section of the paper, typical frequency analyses of the data and insolation reveal characteristic frequencies, and these are associated with the average cycles of the three celestial parameters. My paper treats the behavior of the three celestial parameters as they are in nature. They are quasi-periodic functions of time with wide ranging half-cycles as demonstrated by the numerous graphs in the paper. A temporal comparison between the EDC time series data and the deconvolution model requires that the obliquity and precession index contributions to the insolation be treated as quasi-periodic functions of time. This approach is born out in Figure 12 where I show that the precession index contribution to the percentage change in insolation between maxima and minima correlate
with the temperature trends and interglacial-glacial periods quite well. This result is no accident.

The deconvolution model is an approximate physical description of the insolation. The model is based on the comparatively small role the obliquity plays in contributing to the insolation. It essentially shifts the sun's rays north and south by 2.4 degrees. It changes the angle of the sun’s rays with respect to the vertical and primarily the angular distribution of the insolation over the earth’s surface. That is demonstrated by the derivation of its contribution in Appendix A. The insolation amplitude is primarily influenced by the precession index because of changes in eccentricity and the direction the earth’s axis points during the summer solstice. The timescale of obliquity changes is approximately twice that of the precession index. Therefore, in first approximation the obliquity is constant. The model simply multiplies these contributions to obtain an approximation to the insolation. The insolation magnitude does not matter because the percentage change in the insolation between extrema is calculated and used to compare with the EDC data. The justification for this approximation is born out in the results both in terms of the numerical precision compared to Laskar et al and the qualitative and quantitative behaviors of the PI- and O-wave contributions to the insolation and their comparison with the data.

The paper demonstrates that the precession index dominates the insolation contrary to many papers in the field of Milankovitch Theory. Even Milankovitch believed the obliquity was the dominant effect in the cause of ice ages.

I have no idea what the reviewer means by “a quasi-instantaneous linear forcing represented through the simple Eq. 3.” Linear in what parameter? Equation (3) is nonlinear in the obliquity and precession index contributions to the percentage change in the mean daily insolation between maxima and minima. It’s clearly not linear in time or the three celestial parameters as born out in the subsequent equations to equation (3). What does the reviewer mean by quasi-instantaneous? The estimates of Laskar et al for the insolation and the three celestial parameters have a resolution of 100 years as I state in the paper. Is this what the reviewer means by quasi-instantaneous? What is the point of this comment?

Is the reviewer objecting to the simplicity of the model? What matters are the physical assumptions, the translation of those assumptions into mathematics, and the comparison with the data. That is the scientific method. The paper makes two key assumptions.

The first involves the assumption that the percentage change between successive mean-daily-insolation maxima and minima at 65 degrees northern (65N) latitude during the summer solstice (June) over the last 800,000 years, substantially influenced the prominent features in paleoclimate data. Those features are interglacial inceptions, terminations, and durations, the timing of their recurrence, their classification into two types based on wave interference, etc.

The second involves the deconvolution model, which is a simple physical model as
described above. The rest of the paper describes the consequences of these assumptions and semi-quantitative comparisons to the EDC time series data.

The paper is not a climate model. As discussed in the abstract and the introduction section, the paper explores the Milankovitch hypothesis, which focuses on the mean daily insolation at 65 N degrees latitude during the summer solstice. The issue is what does the behavior of the insolation tell us about the occurrence of interglacial and glacial periods over the last 800,000 years. Are there quantitative and qualitative behaviors of the insolation that account for prominent features in the paleoclimate data such as the timing of interglacial inceptions and terminations, interglacial durations, the recurrence of interglacial-glacial periods, etc. The issue of how the earth’s climate responds to insolation changes is not the purpose of this paper. That is a totally different matter. The specific climate mechanism that causes an interglacial termination is unknown. We do not understand the earth’s climate well enough to predict how it responds to insolation changes and there is no calibration of insolation to temperature. The paper merely is establishing recurring patterns in the behavior of the insolation that correlate with the prominent features in the EDC time series data.

There may be an important point that has been missed by the reviewer. The insolation is inherently a wave phenomenon, which is emphasized in the paper. It is an amplitude modulated wave in time. This wave description involves the interference between the obliquity and precession index waves as defined in the paper that accounts for both quantitative and qualitative features in the paleoclimate data. It appears to be a useful language to describe the prominent features in the paleoclimate data based on the Milankovitch hypothesis. It’s a new and novel contribution to the field.

More generally, this manuscript seems to represent a big step backwards: among other issues, the author implies, without ever saying so, that the climate system on these time scales is linear. But even the original Hayes et al. paper referred to Milankovich "pacing" of glacial intervals, recognizing that the relationship between forcing and response was not that of a linear system. In the intervening time interval, a number of papers (see for example, Tziperman et al., 2006, Paleoceanography, or various papers by Peter Ditlevsen) have dealt explicitly with the anticipated results of forcing a nonlinear system with quasi-periodic deterministic driving. None of this is even mentioned.

I have no idea what the reviewer means by this paper implicitly assumes that the climate system on these time scales is linear. Linear in what parameter? The reviewer seems to miss the point that the paper is not a climate model. The paper is simply exploring the Milankovitch hypothesis through a model that partitions the obliquity and precession index contributions to the percentage change in the mean daily insolation at 65N latitude during the summer solstice. The model provides strong support for the Milankovitch hypothesis. If there is a valid theory or model of the earth’s climate that incorporates a response to insolation force according to the patterns established in this paper, then that would be a
major advance in understanding the cause of ice ages. I know of no such validated theory or model. If the reviewer knows of one, I would appreciate a reference.

The paper is really a collection of wiggly lines. But a huge literature exists on the response of linear and nonlinear systems to deterministic narrow-band (in the Fourier sense) forcing. (Studies of ordinary ocean tides are one much discussed, analogous, application dating back to the 19th Century.) The author repeatedly invokes "correlation" between two wiggly lines---all based on some perceived visual resemblance. But the human eye is notoriously poor at separating apparent patterns from real ones (a branch of psychology known as pareidolia), and for that reason, specific statistical tools have been developed to obtain objective tests. It is a truism of data analysis that even two unrelated records will show a correlation, both numerical and visual, and hence a level of no significance is *always* necessary. In most cases here, one wonders what would have led the author to reject his own hypothesis of apparent correlation? The model being used is fully deterministic and the distribution e.g., of zero-crossing times, is also deterministic unless the system is nonlinear and chaotic. Deviations from claimed correlations are explained away by arm-waving stories (lines 517+ are one example). The use of half-cycles, rather than the usual use of frequencies, here would imply some kind of obscure rectification mechanism at work. Furthermore, the paper takes no account of any kind of noise in the ice core records, including issues of dating.

I cannot make logical sense of these confounding comments. Are the assumptions of the paper wrong and what specifically is wrong in terms of physics? Is the translation of the assumptions into mathematics and the model wrong and what is specifically wrong? The reviewer seems to be stuck on frequencies as the only approach to understanding paleoclimate data. My paper takes a time series approach because of the quasi-periodic nature of the three celestial parameters and the fact that there is more information in such an approach to compare with data. What is obscure about this?

The paper points out the very limited number of points that represent predictions of the model. There are not enough points to perform a statistical comparison with the data. Furthermore, the history of science is fundamentally a search for patterns and the development of theories to explain the patterns. Would the reviewer consider the experiments of Thomas Young concerning the interference patterns of light as a wave a statistical aberration? Explaining those patterns took about a century through the development of quantum theory that could fundamentally account for those patterns quantitatively and qualitatively. Richard Feynman has hand waving lectures that describe all of this without using a single formula. There is more to a good physical argument than most people think.

My paper is based on two assumptions as clearly described above and in the paper. Celestial behaviors, i.e., physics, are used to translate these assumptions into mathematics to define the model. The approximations are justified quantitatively in the paper based on well-established benchmark computations of Laskar et al. The results are used both quantitatively and qualitatively to identify the prominent features in the EDC time series data. Wave interference, a novel aspect of the paper, is used to interpret the data. The model accounts approximately for the recurrence of interglacial-glacial periods over the last 800,000 years, temperature trends, the timing of interglacial inceptions and
terminations, the duration of interglacial periods, the classification of interglacial periods into two types, and quantitative estimates of interglacial terminations. All interglacial terminations consistently occur in the same manner over the last 800,000 years.

Smaller issues:

the 65° N dependence is invoked without any discussion of its relevance, particularly to an individual ice core.

Many (A. Berger et al) have explored the relationship between insolation predictions at 65°N during the summer solstice from celestial mechanics and their correlation with paleoclimate data. This paper simply uses EDC data, which is a temperature model rendering of ice core data. There is remarkable consistency amongst the various paleoclimate data sets, however there are timing issues as I point out in the paper.

No error bars are placed on any of the ice core timings (e.g., Fig. 1)

Several figures refer to curve fits to data points (e.g., line 150), but with no specification of what constituted those curves.

As pointed out in the paper, the EDC ice core data is a temperature rendering of the paleoclimate data published on the NOAA site as referenced. The timing issues with these data are well known as I indicate in the paper. The issue is how well can a model account for the prominent features in the data. Expecting precision from this data is inconsistent with the methods used to infer it. At best, a model comparison with the data can be semi-quantitative. That is the approach taken in the paper and the results obtained worthy of publication.

The caption under Figure 3 and other Figures explains the origin of the graphs. There is a reference, which directs the reader to a well-established computational tool that is used to create the graph based on the simple definition of what is computed.

Why don't the author's hypotheses operate in the interval before 1MY BP, when the glacial-interglacial intervals are widely believed to have occurred only at ~40KY?

The earth was undergoing a major transition likely due to the Isthmus of Panama (about 2.3 million years ago), which changed the dynamic mixing of the oceans. The average global temperature steadily dropped, and the temperature excursions steadily increased likely due to the increasing accumulation of snow and ice at the poles. Eventually, the earth stabilized into the more recent pattern of ice ages over the last 800,000 years. The
model required to describe the earlier period would be different because the physical circumstances were different.

Was the precession index ever defined? Is A truly time-independent?

It’s defined in the abstract and it’s clearly not time independent because it depends on the eccentricity and precession. Anyone familiar with the subject of this paper and celestial mechanics would know this.

Line 97 and elsewhere, what the author means by aperiodic would be the simple beating of two or more nearby frequencies--each individually periodic.

The paper demonstrates that the three celestial parameters are aperiodic. What two or more nearby frequencies that are periodic is the reviewer talking about?

Line 299. Why is it surprising?

Given the derivation in Appendix A, it is surprising that the obliquity contribution to the insolation is well approximated by the formula provided.

Line 413. What is meant by "judiciously"?

The vertical lines are chosen based on the recurring pattern in the PI-wave.